degree to which it can be distended must be very much greater than those qualities in the cystic duct. If these features be found upon extended observation really to exist in the two ducts, a ready explanation will be found in them of what is actually the fact in my own experience, that the occurrence of jaundice either in the skin or in the urine during and after the passage of gall-stones is of extreme rarity, and not, as has been believed, common. The chief symptom, that of pain, would be explained by the slow passage of the gall-stone through the comparatively rigid canal of the cystic duct, whilst its rapid passage through the easily distended and much larger common duct would not give time, in the majority of instances, for the occurrence of jaundice, which could of course only be produced after the obstruction of the flow of bile through the common duct for some considerable time.

In the case which I have just quoted, sent to me by Dr. Aust Lawrence, the facts that the majority of the gall-stones were of pretty uniform size, and that only in connexion with the passage of the first did jaundice occur, seem to me confirmatory of my views. We may take it, perhaps, that the passage of the first stone, by dilating the common duct, rendered the passage of its successors so easy that no jaundice was produced. The first dilatation of the cystic duct has, on the contrary, had no such effect, for her suffering whilst the last stone was passing a few days ago was quite as intolerable as on previous occasions.

I am, Sir, yours, &c.,

Birmingham, June 8th, 1885. LAWSON TAIT.

ETHER AS AN ANÆSTHETIC.

To the Editor of THE LANCET.

SIR,—At the present moment, when the question of Chloroform *v.* Ether is again occupying a prominent position, the following case may prove of interest, as tending to show that the dictum that ether never kills by stopping the heart is one not entirely to be relied on.

I was called upon to administer an anæsthetic to a middle-aged, fairly healthy female patient, who was to undergo an amputation. I elected to use ether by Clover's method; as a matter of routine I examined the heart, which was apparently normal as regards valves and muscular tone. The patient inhaled the anæsthetic readily, and without the slightest struggle. The operation lasted about three-quarters of an hour, and six ounces of ether, which I have reason to believe was of excellent quality, were used. Towards the end of the operation there was a slight tendency to vomit saliva. Following one of these attempts, four or five minutes after the anæsthetic had been laid aside, and, in point of fact, when the dressings were being put on, I noticed that the breathing was becoming shallow, that the radial pulse, which throughout the operation had been small, had become imperceptible, and that the patient was rapidly assuming an ashen livid hue. An endeavour was made for a moment to excite the heart and lungs by artificial respiration; this had no effect, and matters were assuming a very serious aspect, fæcal evacuations being passed involuntarily. Having some of Allen and Hanburys' nitrite of amyl capsules in my anæsthetic case, I broke one on a pocket-handkerchief and applied it to the mouth and nostrils; after two or three inspirations, shallow as they were, there was a very marked change for the better, and after the lapse of a minute the pulse returned and the patient was practically out of danger.

I venture to look upon this case as an instance of temporary heart failure, differing as it did so markedly from the usual symptoms of spasm of the glottis with which one is so familiar during ether administration. It may, perhaps, be questioned whether the ether was to blame for the accident, or whether the shock of the operation had caused the syncope; judging by the trifling loss of blood and other considerations, I am inclined to consider that this was not the case, but that the effort of vomiting, aided perhaps by the possible passage of acrid fluids from the stomach into the larynx, hindered free respiration for a moment, and caused the already embarrassed heart to hesitate, if not to stop altogether.

The behaviour of the nitrite of amyl under the trying circumstances was in the highest degree satisfactory. Even if breathing should cease in cases similar to this, there seems to be no reason why the drug should not be administered by means of artificial respiration. In future it will be a source of great comfort to me, in giving chloroform or ether, to have at hand, in case of accident, so potent an ally.

I remain, Sir, yours truly,

BERTRAM THORNTON, M.R.C.S., L.R.C.P.

Cecil-square, Margate, June, 1885.

METHOD OF DEMONSTRATING THE PRESENCE OF PNEUMOTHORAX.

To the Editor of THE LANCET.

SIR,—The presence of a pneumothorax is usually demonstrated on the post-mortem table by the sound of the escaping gas on opening the pleural cavity, or by the flaring or extinction of a lighted match held over the opening, or else by the bubbling up of the escaping gas conducted beneath water. The latter method is probably the most satisfactory, all things considered, and certainly so if there be a class to demonstrate to. Better still probably is the following method. A length of ordinary glass tubing is bent into manometer shape; on to the short arm of the manometer a piece of rubber tubing is fitted; the other end of the tubing fits on to a hollow sharp-pointed aspirating needle; some water poured into the bend of the manometer completes the apparatus. If the chest of a pneumothorax be now stabbed with the needle, the rise of the water in the long arm of the manometer at once demonstrates the presence of gas in the pleural cavity, and the difference of level of the fluid in the two arms of the manometer measures the intro-thoracic tension of this gas. The needle may be withdrawn and the same or successive intercostal spaces punctured in turn, each time with a repetition of the demonstration. In puncturing the left side low down the needle may penetrate the diaphragm and enter the stomach; in such case the manometer will record a *plus* pressure, as in the case of a pneumothorax; this must be remembered and guarded against. The advantages of this method are: the readiness with which the apparatus is fixed up and applied, and its fitness for demonstration at a distance; further, that it affords a means of measuring the intro-thoracic pressure.

Clearly the method, if necessary and to the advantage of the patient, is applicable during life; but, however applied, its simplicity is such that it can scarcely be novel, and I must apologise to all those who are in the habit of demonstrating the subject after this fashion. That it cannot be quite common property I judge from the fact that the method is not mentioned in Virchow's "Post-mortem Examinations" or in Orth's "Compendium der Diagnostik," both of them practical books; nor have I found it elsewhere.

I am, Sir, yours faithfully,

H. SAINSBURY, M.D., M.R.C.P.,

June 28th, 1885. Assistant Physician to the Royal Free Hospital.

IMMUNITY FROM INFECTIOUS DISEASES.

To the Editor of THE LANCET.

SIR,—In your issues of April 11th and 18th appeared an article by Dr. George M. Sternberg on the possible explanations of acquired immunity from infectious diseases. There is another explanation besides the three mentioned by the writer—namely, the theory that after an attack of the infectious disease the tissues through which the poison can be supposed to permeate in order to reach the blood (lung or alimentary canal) become impervious to the particular germ, so that the germ is unable to re-enter the blood. This explanation is quite distinct from the theory of "vital resistance," which is the destruction of the poison in the blood by virtue of the vitality of the leucocytes in the hæmic or lymphatic systems. According to the "impermeability theory," a second attack would be due to some local disease removing the impermeability of the tissues conferred by the first attack.

Dr. Sternberg has not given the "exhaustion theory" the consideration it deserves. It is not necessary for the germ pabulum to be entirely removed at the termination of the disease. All that is required by the theory is the belief that the germs have an amount of pabulum in the blood which is not sufficient to allow of the propagation of a large number of germs. We know from the existence of a period of incubation that until the germs have reached a certain number